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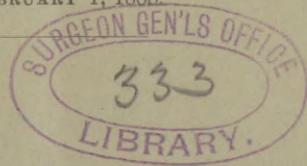
ITS MORTALITY AND TREATMENT.

A STATISTICAL AND RATIONAL INQUIRY.

BY
HENRY HARTSHORNE, M.D.

REPRINTED FROM THE
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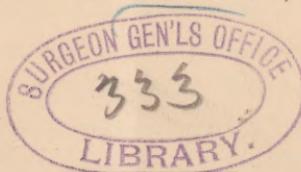
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A STATISTICAL AND RATIONAL INQUIRY
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[Read February 1, 1888.]

MEDICAL practice, in its present state of scientific incompleteness, cannot do without "working theories." But it is of much consequence to discriminate between these and positively demonstrated conclusions. It is only a provisional, not an unalterable, allegiance that is their due. A regency, sometimes a dictatorship, in science, as in government, may be well—on occasion, even indispensable; but a regent must be ready to withdraw when the king comes of full age. King-truths are as yet, of course, few in medicine.

This brief preface is meant to introduce an assertion, which may appear to require more excuse than it affords. I believe that, among the fluctuations in medical practice during the past half century, none may be set down as more largely due to speculative opinion, and less based upon observed facts, than those concerning the treatment of acute inflammatory affections, of which pneumonia may be considered as a type. My medical recollections enable me to traverse a period of quite ex-

tended transition. Under the tuition of my father, who was a pupil of Dr. Rush, I had early personal experience, as well as observation, of so-called "antiphlogistic" practice; which, however, as then existing, had already reached the stage of careful moderation. Then followed a reaction; bringing in an approach to expectaney, with J. Hughes Bennett, and others; and then stimulism, with Dr. R. B. Todd and his followers; and leading the way to the more recent "physiological" rationalism, which is now characterized chiefly by an abandonment of old methods and principles, substituting for them experimentation with powerful agents, tried first in the laboratory upon animals, either in health, or in various conditions of traumatism.

All this is, of course, intended for the advancement of sound medical science; having for its practical end the cure or alleviation of disease, and the prolongation of life. This end justifies the cautious use even of unproved working theories. But, from time to time, it is well to look back, and compare results. Are we making real progress? Is the current "working theory" establishing itself by facts; or is it, instead, obtaining less actual success than that which it superseded?

Coming to the application of this question to pneumonia and allied affections, my opportunities of observation and acquaintance with the transition period, above alluded to, have led to some positive convictions. These have been not long since expressed, and reported in a medical journal,¹ in language so strong as to require to be supported by something more substantial and extended than general impressions and individual

¹ Discussion in Philadelphia Clinical Society; reported in the Philadelphia Medical Times, Sept. 4, 1886.

experience. This feeling has led to the preparation of the present paper.

My *thesis* is this: That the mortality of acute pneumonia, croupous and catarrhal, is decidedly greater now than forty years ago; especially when it occurs in young or middle-aged patients of previously good health; and that this increase of mortality coincides with a prevailing change of treatment, in such a manner as strongly to suggest, if it does not absolutely prove, that the principle of that change is erroneous, either in what it omits or in what it adds, or in both.

Looking for *data* on record to bring this thesis to a definite test, I will revert, in the first place, to a specially comprehensive summary upon the subject, issued while the anti-venesection movement was at its height. This summary was presented in an elaborate article in the *British and Foreign Medico-Chirurgical Review* for July, 1858. Familiar as its matter may have been to medical readers at that time, many physicians of the present generation are likely not to be acquainted with it. A *résumé* of its principal contents may, therefore, be here in place.

The article mentioned reviews especially some papers not long before published, by Drs. Alison, J. Hughes Bennett, T. Watson, W. T. Gairdner, Joseph Bell, and W. O. Markham. But it adds a large number of statistical facts, drawn from various continental as well as British sources. It remarks upon "the almost total abandonment of the lancet of late years." "Change of type," in part, at least, explained this, according to Dr. Alison. "Sounder pathology" was the explanation proposed by Dr. Bennett. Dr. Alison rejoins: "If Dr. Bennett's pathology leads to the belief that the prin-

ciple in therapeutics adopted by the great body of practitioners is false, it is a much better reason for setting aside his pathology than their therapeutics." The great body of practitioners, however, appear to have since been willing, for a time at least, to place under trial the principle of practice inferred from Dr. Bennett's pathology. What has been the result of this trial, is a question worthy of present investigation.

Drs. Watson and Gairdner, in the discussion referred to, essentially agreed with Dr. Alison.

Looking over the facts, the *Medico-Chirurgical Review* gives an account of the practice of several distinguished leaders in the profession. Bouillaud's method of pneumonia, like Cullen's, was bleeding, *coup sur coup*. So was that of Sauvages, Chomel, Louis, and Grisolle. Their employment of the lancet was terribly extreme; with patients advanced in life, it was clearly destructive. Rasori, in Italy, was even more sanguinary in his practice. Laennec was much more moderate with the use of the same kind of treatment, and was more successful.

Skoda, for twelve or thirteen years, only occasionally resorted to bleeding in pneumonia. Dr. Balfour, however, reports him as saying that in 1840 he treated 64 female patients with large bleedings and large doses of tartar emetic, with but 1 death. Yet, in the same year, he had so many deaths in male subjects as to bring his mortality up to 1 death in 8 cases. This was considered by Skoda to be the average result of the treatment of pneumonia under all methods. Balfour, about the same time, watched the treatment of 19 cases in the Homœopathic Hospital, under Fleischmann; 3 died, 1 in $6\frac{1}{3}$. Dietl published an account of his comparatively successful treatment of pneumonia.

without bleeding. But his statistics lack such particulars in regard to his fatal cases as would make practicable a comparison with those of practitioners who have used general or local bleeding. He reported 1 death in $13\frac{1}{2}$ cases treated by diet only.

In the review now cited, many details are given in regard to the management of pneumonia, by Trousseau (who gave tartar emetic in large doses), by Huss, Kissell, Wittich, Cohn, Niemeyer, Wunderlich (who returned to bloodletting), and others; including treatment with inhalation of chloroform by Baumgärtner, Wucherer, Helbing, Varrentrapp, and several other practitioners. Consideration is very properly given to the influence upon the result of age, constitution, season, climate, previous destitution, stage of the attack when treatment was begun, type of the disease, whether sthenic or "typhoid," complications, hospital accommodations, and surroundings.

After the age of sixty years the mortality was notably greater, especially under large and frequent bleedings. As to sex, more women die in hospitals of pneumonia than men, because more old women than old men become hospital patients with that disease. Race, also, has its effect. In the hospitals at Bombay, Morehead and Storer recorded, during ten years, 22 cases of pneumonia in 12,000 European patients, of whom 2 died, 1 in 11; while of natives, there were 313 cases of pneumonia in 26,000 patients, of whom 121 died, 1 in 2.6. Pneumonia has a worse mortality among Negroes, Hottentots, and Malays than among Europeans.

Of the seasons, winter is everywhere the time of the greatest number of cases of pneumonia: in Great Britain 3 to 1 as compared with summer, and with a

still greater proportionate mortality. Statistics referred to in the article now under analysis, make it apparent that "dry cold is the prime cause of pneumonia." It is more frequent and mortal in Paris during March and April than during January and February, because, although not so cold, the former months are drier. While attacks of it are scarce along tropical shores they are abundant and severe in elevated regions; as in the valley of Puna, Peru, at an elevation of 12,000 feet; and at Briançon, France, 4285 feet. The headquarters of pneumonia are said to be Paris, Northern Italy, parts of Germany, and several places in more Northern Europe. More of it occurs in Scotland than in England; and less in the United States than in Great Britain.

Among British troops, the home mortality, as stated by the same authority, 1858, was, from pneumonia, 1 death in 13 cases. In British North America 1 in 11 to 1 in 25; at the Cape of Good Hope, 1 in 12; near the Cape frontier, 1 in 20; near the Mediterranean, 1 in 15 to 1 in 47; West Indies, 1 in 15 to 1 in 28. As these figures must be supposed to be derived from reports of a number of years, they no doubt represent the mortality of pneumonia for a time mostly included in the period of moderate general and local bleeding.

With hospital cases, a very important item is, the frequency of admission late in the attack. Always unfavorable, this is particularly bad under a routine treatment by large and repeated bleedings. No hesitation is called for, about saying that such must be the worst possible treatment at a late stage of the disease.

Records of hospital cases often have their value lessened by the absence of statements as to age, pre-

vious health and habits, stage of the attack on admission, etc. By selection of cases for a special treatment it might be possible to produce a show of results of a quite misleading character.

Much is said by authors concerning differences of *type* in pneumonia; sthenic and asthenic or typhoid cases. There is, undoubtedly, ground for the distinction; but the last named terms are probably often misapplied. Dr. La Roche and others, among them several medical officers of the U. S. Army at Southern stations, have long since established the existence of a malarial pneumonia, the "winter fever" of some localities in our Southern States. A considerable number of such cases are of a "typhoid" type. But I venture to believe that this term has sometimes been applied to cases which were simply not doing well, and thus reached a stage of depression or prostration which might have been prevented by suitable early treatment. Of this, more hereafter.

The question of *change* of type, through longer or shorter periods, is one of difficulty. All that approaches certainty about it is, that, in city populations, the tendency of artificial living, of what we call our civilization, is to lower the average grade of vital resistance. Defect of vital tenacity is especially conspicuous among the poorer and every way least-favored people who furnish the bulk of patients in hospitals. Success with the lancet in treating them should count for more, and that with stimulation for less, than in private practice; because they, less than any others, are likely to be robust enough for bleeding, while by habit with many, and by general condition with nearly all, they are apt

to bear alcoholic and other stimulation better than those who are able to be treated at their homes.

Hospital mortality varies much, even in the same institution at different times. As examples, that of pneumonia was, at the Hotel Dièu, in Paris, 1 in $4\frac{1}{2}$ in 1787, 1 in 7 in 1822, 1 in 9 in 1839, 1 in 7.32 in 1848. In the hospitals of Lyons it was 1 in 13 to 1 in 11 in 1787, 1 in 11 in 1822; at Padua 1 in 12 in 1821; Vienna, 1 in 12 to 13 in 1787, 1 in 6 in 1824, 1 in 7.2 in 1856; in the Edinburgh Infirmary, 1 in $25\frac{1}{2}$ in 1787, 1 in 16.6 in 1817, 1 in 8.2 in 1842. At St. Bartholomew's, London, 1 in $8\frac{1}{2}$ in 1787, 1 in 11.3 in 1819, 1 in 12.6 in 1834. In the Suffolk Hospital, 1 in 50 in 1835. The familiar fact need scarcely be reiterated, that the results are, as a rule, most unfavorable in the large city hospitals.

Making a careful scrutiny of the reported results of different methods of treatment, the *Medico-Chirurgical* reviewer above quoted, observes that "the strong very generally recovered, and that often speedily and well, who were treated by Chomel, Louis, Bouillaud, Briquet, and Grisolle, with repeated bleedings, especially when performed early in the disease." While, no doubt, such extreme treatment killed many weak and old patients, it is added that "a first or even a second bleeding rarely or never produces an injurious effect on cases of pneumonia unless the disease is of a low type, or in an advanced stage, or the constitution is deteriorated by old age, destitution, drunkenness, or disease, especially Bright's disease."

Dr. Bell treated 71 cases of pneumonia in the Glasgow Infirmary; 36 were bled without the loss of one; 6 of

these being bled from the arm, the rest locally, chiefly by cupping. We see that this was, from the standpoint of that period, moderate treatment. Even Dr. Hughes Bennett sanctioned small and moderate bleedings in some cases, particularly to palliate excessive dyspnoea and pain. A number of practitioners have always approved the use of venesection to relieve oppression from distention of the right heart, even at what cannot be called an early stage. The benefit resulting from this practice, with the absence of consequent serious debility, may be carried over for rebuttal of the excessive fear of exhaustion indulged by many in connection with the withdrawal of blood in the early part of the attack.

Allusion has already been made to Dietl's reports of cases, and their statistical insufficiency. He gave some account of 380 cases; 85 treated by venesection, 106 by tartar emetic, 189 by diet only; with a mortality of 1 in 5 of those bled—no doubt, largely and repeatedly. 1 in $13\frac{1}{2}$ of those treated only by regulation of diet, without bleeding or medicine. Later, he treated 750 cases without bleeding, other particulars not given with a reported mortality of 1 in 10.8.

Dr. Hughes Bennett recorded a mortality of 1 in 21.7, without bleeding. His patients were comparatively young; their average age, 31 years.

The result of non-bleeding in pneumonia in the Vienna Hospital, in 1856, was very unfavorable; the deaths being 1 in 4 among the males, and 1 in 2.7 among the females. The general conclusion drawn by the *Medico-Chirurgical* reviewer from an immense mass of facts is that, while indiscriminate and repeated bleedings may often do harm, both experience and science

appear to press upon us the propriety of “the discriminating practice of moderate and early bleeding, general or local, in cases of more or less sthenic inflammation, and of refraining from it altogether in asthenic cases, whether as regards the character of the disease or the constitution of the patient.”

The mass of facts thus alluded to includes published reports of between forty and fifty physicians in Great Britain and Ireland and different parts of Europe, giving account of 11,627 cases of pneumonia. Of these, 2751 cases were treated with bleeding, and 8876 without it. Among the latter, however, 452 cases had exceptional modes of medication, in the use of chloroform, lead, copper, or iron. Deducting these, the statistical comparison, so far as the figures have value, rests between 2751 cases treated with, and 8424 cases without bleeding.

I have carefully analyzed the tables in which these statistics are given, with the following as the principal results. First, the more remarkable instances on both sides may be mentioned. Lacaze reported, under large bleedings, in pneumonia, the loss of but 1 in 42; Joseph Bell (before mentioned), with bleeding, no death in 36 cases; Troussseau, without bleeding, but with tartar emetic, 1 in 26; Bennett, without bleeding or tartar emetic, 1 in 21.7. Skoda’s experience in one year has been referred to; yielding under bleeding but 1 death in 64 women. Less stress, for our present purpose, may be laid on these exceptional figures, than upon those which deviate less from the general aggregate; but, so far as they go, they do not oppose the conclusion above cited on the subject.

The essential reported facts concerning these 11,175 cases may be set forth as follows:¹

With bleeding, either often, a few times, or once, with or without tartar emetic, altogether, 1 death in 11.1 cases.

Without bleeding, under all treatments except with opium,² 1 in 9.7.

Large and repeated bleedings alone, 1 in 11.6.

Bleeding a few times, or moderately, alone, 1 in 12.3.

Bleeding and tartar emetic, 1 in 12.56.

Tartar emetic, no bleeding, no opium, 1 in 11.3.

Opium, without bleeding, 1 in 3.3.

Tartar emetic and opium, without bleeding, 1 in 3.8.

Chloroform³ (9 practitioners, 280 cases), 1 in 23.9.

It is, of course, very desirable, in the present inquiry, to make as full a comparison as is practicable of the mortality of pneumonia in this country during what may be called, for convenience, the bloodletting period, and that which has followed the change of practice, dating about 1856, '57, '58. Not many reliable sources exist for such information. I know of none more definite than the returns of the medical officers of

¹ In a few instances I have had to be content with an approximation, as when a certain number of cases are reported, with *no deaths*; Dr. Bell's, at Glasgow—*e. g.*, 36 cases, treated with bleeding, and no death. The least room for error here has seemed to me to exist when it is supposed that the *next* case might have been fatal; estimating, for example, Dr. Bell's rate as 1 death in 37 cases. This being done alike on *both sides* of the account (under bleeding and non-bleeding treatment), can hardly have materially influenced the total result. Throughout the consideration of these statistics, I have leaned toward a large allowance in favor of the side opposite to my convictions. On behalf of these a much stronger case might have been made if my object had been anything but the actual truth.

² Except, also, with chloroform, lead, iron, and copper, as before stated.

³ Not included in the 11,175 cases in which have been reckoned the others of this table. Considering the small mortality occurring under this treatment, it seems remarkable that it should have dropped out of view in latter years.

the United States Army. According to these,¹ between 1840 and 1854 there were in the army 1416 cases of pneumonia, with 127 deaths, 1 in 11.15; between 1855 and 1859, 657 cases, with 97 deaths, 1 in 6.67. From my standpoint, there may be seen in this increase of mortality during the second period, a measure of the influence of the then proceeding change in the prevailing "working theory" of practice.

This view receives strong confirmation from recent statements concerning the mortality of pneumonia. In the Philadelphia *Medical News* of December 11, 1886,¹ it is editorially mentioned, that:

"The rate of mortality in the large general hospitals in this country is rarely below—more often above—twenty-five per cent., which represents about the average death-rate from this disease in the Northern and Southern armies during the Civil War. In this country extensive statistics of pneumonia in private practice are not available; but in the recent returns of the Collective Investigation Committee of the British Medical Association the mortality was eighteen per cent."

As to the general fact concerning the prognosis of pneumonia forty years ago, few men have been more competent to pronounce an authoritative judgment than the late Dr. George B. Wood, of Philadelphia. He wrote thus, in the first edition of his *Treatise on the Practice of Medicine*,² published in 1847:

"In cases of primary pneumonia, of the common or lobar kind, occupying only a portion of a single lung, occurring in persons of a good constitution, and without complication of

¹ Medical Statistics of U. S. Army, 1839-1855, and 1855-1859.

² Page 660.

³ Vol. ii. p. 41.

any kind, there is every reason to hope for a favorable issue. Cases of this kind almost always end in recovery under proper treatment, and not unfrequently even without remedies, or with such as are improper. The disease appears to be remarkably mild between the ages of six and twenty-one. Of forty cases observed by Dr. Gerhard and M. Rufz in the Children's Hospital at Paris, occurring in children from six years old to the age of puberty, only one terminated in death."

Dr. Wood goes on to remark upon the well-known increase of danger produced by previous debility and old age, in double pneumonia, and in hospital patients, as compared with those seen in private practice.

The field of observation of Dr. Wood was, besides his private practice, chiefly in the medical wards of the Pennsylvania Hospital; his conclusions, however, being strengthened by a wide and thorough acquaintance with the medical literature of his day. His practice, with which, as resident physician in the Pennsylvania Hospital, from 1846 to 1848, I had ample opportunity to become familiar, was what might be called moderately "antiphlogistic;" consisting, usually, in cases not previously debilitated, and occurring in early or middle life, of bleeding, neither excessive nor frequent; early and thorough evacuation of the bowels by an active cathartic; small doses of tartar emetic; at night one dose of calomel, opium, and ipecacuanha, and the same, at a later stage, if improvement was slow, repeated at intervals during the day. There was reason for satisfaction with the results of this treatment; which was differed from on the part of the other attending physicians of the same hospital at that time, chiefly in the direction of less activity, though not to such a degree as to merit the name of expectancy. Many years

afterward, in conversation with Dr. Wood, he recalled the cases thus treated; especially one of aggravated double pneumonia, which recovered after early and free venesection.

I have lately investigated the records of the Pennsylvania Hospital, as a representative institution, with a medical staff excelled by none in ability and reputation, to ascertain the number of cases and deaths from pneumonia, at three periods: 1845, '46, and '47; 1865, '66, and '67; 1884, '85, and '86. The figures obtained by me are these: in 1845, '46, and '47, of pneumonia and pleuro-pneumonia, 16 cases and 1 death (which occurred on the day after the patient's admission); 1865, '66, and '67, pneumonia and pleuro-pneumonia, 27 cases and 5 deaths (one of them on the day after admission); 1884, '85, and '86, of pneumonia, catarrhal and croupous, and pleuro-pneumonia, 93 cases and 29 deaths (one on the day after admission). For the first period, therefore, we have a mortality of 1 in 16, or $6\frac{1}{4}$ per cent. For the second period, 1 in $5\frac{4}{5}$, or $18\frac{1}{2}$ per cent. For the third period, 1 in 3.2, or more than 31 per cent. It needs to be remarked that I have gone carefully over all these cases, as recorded on the books of the hospital, so as to eliminate any that could be suspected of a tuberculous character, as being really cases of phthisis instead of acute pneumonia; the diagnosis made and entered at the time of the *death* of the patient being always taken in the fatal cases.¹

¹ A few cases not fatal but becoming chronic are included in this summary: 1 in 1884, in the hospital seven months and discharged cured; 1 in the house five months, 1 two months, and 1 over one month; all discharged as cured. In 1885, 1 in the house nearly three months, and 1 two and a half months, discharged as cured, and 3 who left the house marked *improved*. In 1886, 1 cured after two months, and 1 after nearly one month in the hospital. So far as any

Although the care taken in obtaining the above enumeration of cases and deaths from the records of the hospital was such as to make it difficult for me to suppose it not correct, my attention has been called by Dr. Osler to another computation of the statistics of pneumonia in that institution, recently made. According to this estimate,¹ there were in 1845, '46, and '47, 25 cases of pneumonia with 4 deaths, 16 per cent.; in 1865, '66, and '67, 29 cases and 7 deaths, 24 $\frac{1}{2}$ per cent.; in 1884, '85, and '86, 88 cases and 30 deaths, 34 $\frac{1}{2}$ per cent. So far as these periods are concerned, the comparison still bears out my assertion of the mortality being latterly more than double what it was forty years ago. Taking in longer periods, the statistics now referred to indicate a much less considerable difference, in that hospital, in the mortality between 1840 and 1850 from that between 1880 and the present year; but still the difference is enough to sustain the statement of a decided increase.² If we include in our account the record of Suffolk Hospital, England, in 1835, of 1 death in 50 cases, and accept the computation of the last-mentioned results in the Pennsylvania Hospital for 1845, '46, and '47, viz., 1 death in 6 $\frac{1}{4}$ cases, or 16 per cent., the mean hospital ratios of the two periods will abundantly show

doubtfulness of diagnosis might attach to these cases, their inclusion tends to better the record as regards mortality; by increasing the whole number of estimated cases of pneumonia not fatal, in the last of the periods considered.

¹ Probably the difference may, at least in part, be accounted for by the inclusion, in my cases, of those only in which the diagnosis of pneumonia was entered at the time of the *death* or *discharge* of the patient, as well as at the time of admission. Modification of the diagnosis is sometimes made necessary during the development of cases.

² The statistics mentioned give an excess of 3.2 per cent. of the mortality from 1882 to 1887 over that from 1842 to 1851; the latter being more than 31 per cent.

that the mortality in such institutions is much greater now than formerly.

As to the general mortality in the community from pneumonia during the same period, inquiry at the Health Office in Philadelphia failed to elicit statistics prior to 1860. The recollection of any physician long resident in this city may well give confidence to the belief that the number of deaths from pneumonia in proportion to the deaths from other causes, has largely increased. Dr. Lewis P. Gebhard, a practitioner of the old school, of large experience, beginning early in this century, told me near the end of his life that he never lost a patient with uncomplicated pneumonia. Dr. A. L. Loomis states¹ that, in New York, the average ratio of mortality from pneumonia to that from all diseases was 15.2 per cent. greater between 1859 and 1877 than between 1840 and 1858. In several weeks during the spring months of the year 1887, the deaths from pneumonia in Philadelphia numbered between 30 and 50 or more in each week; in the last two weeks of that year they were 39 and 47 respectively. More than 600 deaths from pneumonia occurred in this city in the first six months of 1887, in *minors*, besides a large number in adult persons, which total number I do not have at hand. A prevailing epidemic of measles may have aided in swelling this list; but my conviction needs also here to be expressed, that the mortality of measles, with or without the complication or sequela of pneumonia, has undergone an increase of late years, in consequence of the change, adverted to in this paper, in prevailing methods of practice.

¹ American System of Practical Medicine, vol. iii., article, Croupous Pneumonia.

How, then, in view of the facts thus brought forward, does the case stand? Accepting as authoritative the statements of *The Medical News*,¹ already alluded to, we find that the recent or present mortality of pneumonia is, in the hospitals of the United States, 25 per cent., or 1 death in 4 cases; according to the returns of the Collective Investigating Committee of the British Medical Association, 18 per cent., or 1 death in 5.55 cases.

Turning back to the times preceding the alteration in practice which began shortly after the middle of this century, the record is substantially as follows: Skoda's estimate of average mortality of pneumonia, 1 death in 8 cases. Balfour's report of a homœopathic hospital (treatment probably *nil*) 1 in $6\frac{1}{3}$; Dietl, with diet only, 1 in 13.5; British Army, 1 in 13, 1 in 11, 25, 12, 20, 15, 47, 15, and 28—*mean*, 1 in 20.66; hospitals in Europe, 1822 to 1856,² 1 in 9, 7.32, 11, 15, 6, 7.2, 8.2, and 12.6—*mean*, 1 in 9.54.³ United States Army, 1840 to 1854, 1 in 11.15. As a total result, we may fairly estimate the average mortality of pneumonia, in the second quarter of the present century, as not more than 1 death in 11 or 12 cases, or from 8 to 9 per cent.; less than half the present estimated mortality of the same disease, according to the British Collective Investigation Committee, viz., 18 per cent. Moreover, it cannot have escaped the observation of many physicians, that the deaths from acute pneumonia, of late years, include a considerable number of men and women in the prime of life, who

¹ December 11, 1886, p. 660.

² 1787 (already quoted) is omitted, because, although percussion dates from Auenbrugger, 1761, there was less certainty of physical diagnosis before Laennec's introduction of auscultation, about 1818.

³ Suffolk Hospital is here omitted, its exceptionally small mortality (1 in 50) being reported for only one year, 1835.

were previously in good health; a much larger proportion of these, I venture confidently to assert, than was anywhere known half a century ago.

Our next question is, What is the cause of this increased mortality? Several hypotheses concerning it are conceivable.

First, the records may be imagined to have undergone modification in consequence of improved methods of diagnosis. There is no ground for this supposition. In the decade from 1840 to 1850, physical diagnosis (as well as symptomatology) was, at least in regard to pneumonia, well understood among those whose reports have furnished the material for this investigation. So far as it may have been otherwise, it is well known that the tendency formerly was to include under the designation of pulmonary inflammation at least as many cases which would now be recognized as acute tuberculosis, as of any affection whose result might lower the rate of mortality.

Secondly, Does Dr. Alison's theory "of change of type" account for it? This may be hypothesized as either a change in the character of the disease, or in the average constitution of those subject to it. Neither of these appears to me to be so proved as at all to meet the requirements of the case. Pneumonia varies with locality, season, etc., but within not wide limits. Besides the allowable discrimination between cases which are open, active, sthenic, and asthenic or typhoid (as well, of course, as complicated cases), it is essentially the same disease that it was fifty or a hundred years ago.

As to a change in the average human constitution, admission has been made of the possibility, that artificial modes of living, with greater crowding of population,

may have somewhat reduced the average vital tenacity of men. But, against this, we have to put the much improved sanitary conditions of hospitals, and, nearly everywhere, of private dwellings, even of the poor. If, moreover, the increase of mortality from pneumonia of late years had coincided with the prevalence everywhere of the treatment of Chomel and Bouillaud, by bleeding *coup sur coup*, or of Rasori, by large bleedings, with half grain or grain or more (we read of six or eight grain) doses of tartar emetic, we might well believe the inappropriateness of such extreme measures to the present average man to have been shown. But the fact is, that the time of supposed lowering of constitutional resistance is also that of the withdrawal of reducing measures of treatment for inflammation; so that they, at least, cannot be charged with the result alluded to. My conviction is that the conception of change of type in the common constitution of men is exaggerated and misconstrued, if it is considered to account for a larger number of deaths from pneumonia (especially in private practice and with patients in the prime of life) under a predominantly stimulant treatment now, than occurred forty years ago under bloodletting and measures allied with it in practice.

We are brought thus expressly to the question of the relation between a change of prevailing treatment and the existing increase of mortality from pneumonia. The fact of the change of treatment is patent, and will, I presume, be conceded by all. It may be readily verified by reference to any of our medical periodicals;¹ which,

¹ See, for examples, Medical News, editorials, December 11, 1886, and March 19, 1887; Hospital Notes, Treatment of Pneumonia in New York, Philadelphia, and Massachusetts hospitals, March 5, March 12, and March 19, 1887.

more nearly than the text-books in use, reflect recently current views and methods of practice. We may safely characterize the now prevailing method or theory of treatment of pneumonia, in hospitals¹ at least, as exhibiting the following features: 1. The practically universal omission of venesection, and the very rare local abstraction of blood. 2. The general disuse of active cathartic medicines in the early stage. 3. By many, perhaps a majority of physicians, the early and continued use of alcohol, to the extent of from two to twelve or more fluidounces in twenty-four hours; quinine, mostly in ten to twenty grain doses once or twice daily; opium, or morphia, from the start, averaging perhaps morph. sulphat. one-twelfth to one-eighth grain every three or four hours; with deviations from or additions to this general plan, by the use of antipyrin, antifebrin, aconite, digitalis, etc.; and warm applications, as poultices or cotton batting, to the chest, or sometimes counter-irritation with oil of turpentine, etc. The treatment of croupous pneumonia advised by Dr. A. L. Loomis, in his article in vol. iii. of the *American System of Practical Medicine*, is essentially the same in principle as that now described.

¹ A resident physician in one of the oldest and best large hospitals in America, writes, in response to my inquiry concerning practice in pneumonia in that institution: "Venesection is practically never employed. . . . Local depletion is rarely employed. Salines or other cathartics are used as indicated by inaction of the bowel. Tartar emetic is not used. Nitrate, citrate, or acetate of potassium is never or very rarely used. . . . Quinine is used in doses of from six to twelve grains daily from the first; preferred in large doses near together. Alcohol is used when the pulse begins to weaken, in amounts varying from two to twelve fluidounces daily, or more in rare cases. Opium is used to quiet restlessness or irritative cough. . . . Carbonate of ammonium is used freely when the pulse has weakened, five or ten grains every two or four hours." Mention is made also of poultices, cotton or woollen jackets, etc.

What reasons can be assigned for this so considerable change in practice? We may briefly refer to three lines along which its justification may be proposed and attempted.

1. Is Dr. J. H. Bennett's view tenable, that a corrected knowledge of the pathology of pneumonia, regarded as a local inflammatory affection, should induce a change in its treatment? I certainly think not. Dr. Bennett's attention, as a pathologist, was almost exclusively given to the history of the exudation in inflammation, as Virchow's has been largely to cell-proliferation, and Cohnheim's to the migration of leucocytes. But no reasoning or observation, microscopic or otherwise, however it may add certain minutiae, and may clear up, for example, the distinction between croupous and catarrhal pneumonia, can do away with the palpable main facts, that were as well known half a century ago. We have still the first stage, of congestion, and the second of red hepatization of the lung. We have, in other words, *stasis*, with interrupted nutrition, at the focus of inflammation; *concentric hyperæmia* there, and general vascular excitement of the system; then *exudation*, the nature of whose changes chiefly determines the final result, at least so far as the lung is concerned. The increased vascular tension following a local obstruction to the circulation, gave the indication for measures of relief of that tension in the older practice; and that indication still remains, in the presence of the most advanced researches as to either the crass or the minute morbid anatomy of pneumonia.

2. But a pathological view of a different kind has latterly claimed much attention in connection with the asserted microbial causation of pneumonia. The idea

that it is a constitutional, not primarily a local, disease, has found favor for a number of years with a few nosologists.¹ From this view, as a general statement, I dissent altogether. Many observations, since those of Friedländer, make apparent the occurrence of bacilli and micrococci in connection with inflammation of the lungs. Weichselbaum, of Vienna, reports the observation of a diplococcus, a streptococcus, a staphylococcus, and a bacillus, in a considerable number of cases. Sternberg,² while denying the pathogenic character of Friedländer's bacillus, adduces the evidence of Fränkel and others to sustain his belief that the micrococcus first found by him in human saliva is the microbe most frequently present in the fibrinous exudate of croupous pneumonia. Jaccoud has expressed a favorable view of this opinion (*La France Médicale*, May 7, 1887). Two questions need here to be considered: Is the essential dependence of the disease on the microbes demonstrated; and, if it were so, what relation should such a causation have to the treatment? The second of these questions only, is of direct importance to our present subject; but a few moments may be given, in passing, to the first.

In this, as in other cases of microbic investigation, it must, of course, be remembered (although not rarely of late almost if not quite ignored) that coincidence does not at all prove causation. But, in the instance of pneumonia, there are large series of facts right in the

¹ Dr. A. L. Loomis, for example, definitely propounds that opinion, in the American System of Practical Medicine, vol. iii., article, "Croupous Pneumonia."

² President's address before the American Public Health Association, Memphis, 1887.

way of accepting the theory of its dependence on microbial causation. Proverbially, promoted as it obviously is also by bad air and close living, it stands in an immediate relation to cold. I have cited already from an English authority a strong statement, based on extensive records in Great Britain and elsewhere, that *dry cold* is the great factor in the production of pneumonia.

Jaccoud, in a communication to the Académie des Sciences,¹ presents cogent demonstrative reasons adverse to the notion that pneumonia can be exclusively dependent for its causation on microbial infection, its usual primary cause being exposure to cold, especially dry cold; while he considers it not improbable that there may be a contributive morbific action of the pneumococci, affecting to an important extent the character of the disease. Jaccoud concludes that a rigorous analysis of the observations which he records shows recent microbial developments not to be subversive of anciently accepted truths, and that "progress should be sought in the productive reconciliation of traditional medicine with the discoveries of microbiology." In this recognition of a right relation of harmony between old and new facts and ideas, Jaccoud gives an example of philosophical method, such as is especially applicable to the inquiry in which we are now engaged.

Dr. Henry B. Baker, of the Michigan Board of Health, has recently made the evidence upon the causation of pneumonia definite and full. He has collected and compared more than 27,000 weekly reports from different parts of Michigan concerning diseases, and over 120,000 corresponding meteorological observations.

¹ *La France Médicale*, May 7, 1887, page 645.

His conclusion, thus strongly supported, is, that the prevalence of pneumonia is directly and constantly associated with a low temperature and a dry condition of the atmosphere.¹

But if it were even shown that, notwithstanding such facts, pneumonia is either dependent upon, or is in a certain number of cases produced by, microbes: What effect should this have upon our views of its treatment? Of course it would be, in that case, reasonable to wish for some effective microbicide treatment. I cannot accuse any medical thinkers of such a fallacy as is involved in the conclusion, that, because a disease is believed to be of bacterial origin, therefore no treatment other than that which is capable of destroying bacteria can be suitable for it. But some ground does exist for a fear that the present enthusiastic study of pathogenic bacteriology has engendered, or promoted, a therapeutic apathy, which already had some encouragement in the despair of old methods existing in a certain number of minds. Yet to do the best we can is worth while, in so serious a matter as the practice of medicine, while waiting and hoping to do better in the coming time.

3. Excluding, then, the above hypothetical explanations as insufficient, my conclusion remains, that the change of practice referred to has had its origin in a

¹ Dr. Baker's theory for the explanation of this association is plausible. He ascribes it to the excessive exhalation of moisture from the lungs in breathing an air whose humidity is far below saturation; chloride of sodium being thus left (being not volatile) to accumulate in the lungs from the blood, in sufficient quantity to produce irritation and inflammation. Dr. Baker refers in confirmation of this theory to the well-known observations of Beale and Redtenbacher, of the presence of chlorides in pneumonic sputa, their absence at the same time from the urine, and their returning presence in the urine during convalescence. See *Science*, August 27, 1886, and *Annals of Hygiene*, December, 1886; also, *Proceedings of American Climatological Association*.

fluctuation of opinion, not justified by facts or reasoning, upon some fundamental principles of general therapeutics.

Primary among these is that which concerns the value of bloodletting, general and local, as a remedy. My conviction in regard to this has been implied already; and it would make this paper quite too long to extend its discussion. It is needful only again to say that the utility of the withdrawal of blood in giving prompt and often permanent relief to excessive vascular tension, such as exists in the early stage of acute pneumonia and allied affections, has been amply *proved* to me, on my own person, and on many patients. The importance of this excessive vascular tension, in the early part of severe acute local inflammations, does not seem now to be generally appreciated, as a pathological fact. Much less, at the present time, do many practitioners appear to be ready to recognize the potency of relief for it obtainable by the early and moderate abstraction of blood, in vigorous patients and sthenic cases.

A large part of this paper was written before I had opportunity to read Dr. MacDougall's article "On the Value of Bloodletting," in the July number, 1887, of the *American Journal of the Medical Sciences*. He speaks at some length of high arterial tension, especially in connection with renal disease. The prevailing tenor of his essay is very sound; but he appears to me to fall short on the subject now under discussion, when he says of pneumonia, that the occasions are "very few, and very far between, in which bloodletting is demanded." While venesection may probably be dispensed with in a majority of cases, early local bleeding, by cups or leeches, will do good in the greater number, when the

health of the patient has previously been good, and the age is less than sixty years.

Some other writers, besides Dr. MacDougall, have lately dealt with the question of the treatment of pneumonia; notably, Dr. J. L. Dorset,¹ of Virginia, who favors a return to the older methods; but the subject has not been a frequent one in recent medical discussions.

There is a condition closely allied, at least from the standpoint of practical therapeutics, to high vascular tension, and mostly its immediate sequel or effect, which does not, in the pulse or otherwise, present the same signs. This is cardiac, vascular, and organic—in a word, general or systemic *oppression*. We meet with this in the early stages of many attacks of acute disease, in which a kind of debility is present, very different in origin from the debility of exhaustion, and requiring a different treatment. An extreme example of this, occurring in my own practice, was that of a boy twelve years of age, whom, during the epidemic of 1849, I bled in an attack of cholera. He was in incipient collapse, and reacted while the blood was flowing from his arm; going on to recovery. Elementary, obvious and ancient as the lesson of such cases is, as to the practical difference between *oppressive* and *exhaustive* debility, it,

¹ Virginia Medical Monthly, 1886-7, xiii. p. 857. For other articles on the subject, see C. F. Knight, London Lancet, 1887, i. p. 207; Alexander, Lancet, 1887, i. p. 977; Claiborne, New England Medical Monthly, 1886-7, vi. p. 287; Phillips, N. Y. Medical Journal, 1887, xlvi. p. 350; Records, New England Medical Monthly, 1886-7, vi. p. 440; W. Pepper, Medical and Surgical Reporter, 1887, lvii. p. 1; also a discussion in the Transactions of the New York Medical Association, published in 1886, ii. p. 128. Among the modes of treatment commended by some of these authors, is the use of turpentine, by inhalation and by the stomach, and that of ergot and of salicylic acid internally.

or at least its application, has apparently dropped for a time out of the current therapeutics of the present day.

Almost as ancient, though less obvious, has been the evidence of the value of general or local bloodletting as an *eliminative* agency. In the applicability of this, much more extensively than has yet been realized in practice, there is reason for great confidence. Animals bitten by venomous serpents have been known to recover speedily after the abstraction of blood. The microbial origin of diseases points right in this direction; since it is the present accepted view that pathogenic bacteria usually act, not as wasting parasites, but by means of the ptomaines and leucomaines which they produce. Removal of a certain portion of tainted blood must favor the relief of the system from the effects of poisons contained in it; concentrated nourishment at the same time favoring the reproduction of healthy blood. Even the men of the last generation knew the value of "bleeding and feeding" in some cases.

Parallel to the above-mentioned change is the virtual disappearance from practice of the old procedure of active catharsis in the early stage of pneumonia and allied affections. This omission is a loss, not a gain. Lawson Tait has recently shown the practicability of treating safely with purgatives even (surgical) peritonitis; the one disease in which, most of all except in enteritis, we have been especially taught their prohibition. Saline cathartics, like bleeding, lessen vascular tension, moderate excessive temperature, and promote the elimination of morbid elements from the blood. The late Dr. Joseph Hartshorne was accustomed to say that a principal reason for the occurrence of troublesome sequelæ after scarlet fever, measles, smallpox, etc., was

“the neglect of proper evacuation in the early stage of the disease.”

In 1860, it fell to me to review, in the *American Journal of the Medical Sciences*, the “Clinical Lectures on Certain Acute Diseases,” of Dr. R. Bentley Todd, of London. Dr. Todd, in that book, refers to “a notion prevalent in the schools,” that “certain acute and sthenic inflammations are attended with an undue exaltation of the vital forces, both local and general, and that these must be reduced before the inflammation will yield.” On this statement I then remarked :

“We are not aware of any form of disease whatever, in which it is a legitimate object of the physician to reduce, by treatment, the strength or ‘vital forces’ of the patient. But this does not prevent our occasional resort to the careful and moderate abstraction of blood, for the purpose of *restoring the balance of the circulation* in its disturbed states. And we believe it to be a mere imagination that the withdrawal of a few ounces of blood must *necessarily*, in *all cases*, make the patient weaker than he would otherwise become with the progress of his malady. There is no experience more certain than that, in cases of high sthenic inflammation, in the early stage, blood-letting promotes diaphoresis, diuresis, and the action of the bowels; the *very objects* which Dr. Todd sets before us in his eliminative treatment.”

While Dr. Todd was to some extent an innovator, in his advice to give alcohol early and freely in all acute diseases, to meet the tendency to debility, he saw, as a clinical observer, the importance of attending to the eliminative functions of secretion and excretion. He said, in the work just mentioned :¹

¹ Clinical Lectures, etc., Philada. edition, 1860, p. 275.

“ When recovery takes place in pneumonia, there is always some kind of *critical* evacuation, either by sweats, or by urine, or by the free discharge of a purulent fluid from the bronchial tubes, or by pulmonary abscess.”

Dr. Todd favored the occurrence of such critical evacuations by occasional purgings, and by large doses of the acetate or citrate of ammonium ; avoiding opium, at least in large doses, throughout the treatment.

This avoidance I believe to be an important point of practice. There is no valid indication for, and there is much against, the use of opium at an early stage of pneumonia. In large doses, it is then almost certain to do harm ; especially by lessening the tendency to free bronchial secretion, which ought to be encouraged. The language of Dr. C. F. Knight, of Mercer’s Hospital, Dublin,¹ is hardly too strong, when he insists on “ avoiding opium in all forms of pulmonary disease ; ” at least if we limit the application of this *dictum* to the early stages of acute inflammatory affections of the lungs. In paroxysmal dyspnœa, opium or morphia is often useful.

Saline “arterial sedatives” and promoters of secretion, viz. : nitrates, acetates, and citrates, of potassium or ammonium, and, in violent cases, the tartrate of antimony, were formerly used with much confidence in the treatment of the early stage of acute inflammatory affections. These seem to be now, in prevailing practice, substituted by quinine, antipyrin, antifebrin, etc., for the express purpose of reducing excessive temperature. Has this substitution been a gain ? I believe not. The agents mentioned do not equal those of the mineral

¹ London Lancet, 1887, i. pp. 207, 208.

group displaced by them, in *increasing secretion* from the kidneys, skin, and bowels. They seem to act rather by force, as it were, chiefly through the nervous system, in depressing temperature. More harm than good, on the whole, is to be expected from them in pneumonia. Their utility in neurotic affections is attested by many observers. Dujardin Beaumetz, among others, while discrediting the asserted antipyretic value of acetanilide or antifebrin, asserts its positive merits in the treatment of epilepsy. Antipyrin has been found of decided service as a hypnotic in insanity,¹ in some forms of cephalgia, etc., and as an antispasmodic in whooping-cough, as reported upon recently by Dr. J. P. C. Griffith, and others. This is a different *role* from that of medication likely to be available in the therapeutics of acute and uncomplicated visceral inflammation. In Dr. Pepper's interesting case of recovery from pneumonia in a child, under the use of antipyrin (reported last year in the *Med. and Surg. Reporter*), it appears to me that it was the special neurotic complication of a tendency to convulsions which received benefit from the antipyrin, rather than the pneumonia itself.

Testimony is accumulating as to the failure of the use of quinine in antipyretic doses in the treatment of pneumonia. Drs. Bartholow and Osler, in Philadelphia;² Drs. Kinnicutt,³ Ripley,⁴ Putnam Jacobi, Castle, Billington, and Emmett Holt, in New York; and Drs. Shattuck and Minot, of Boston,⁵ are among those who dissent from the now common approval of this point of practice. Pilocarpin, from its degree of action in in-

¹ *Gazette Hebdomadaire*, Dec. 28, 1887.

² *Medical News*, March 5, 1887, p. 261.

⁴ *Ibid.*, Jan. 29, 1887, p. 188.

³ *Ibid.*, March 12, 1887, p. 290.

⁵ *Ibid.*, March 19, 1887, p. 318.

creasing secretion, especially that of the skin, promises more than the other agents mentioned; and some favorable reports of its use in pneumonia have been published.

Reference has been made on a previous page to the debility, of more than one kind, which may occur in cases of pneumonia, as in other severe acute diseases. Clinical writers often speak of *heart-failure* as a source of danger. Undoubtedly it is so. But in robust patients, under middle age, the *cause* of heart-failure, at least in the early stage, which is to be most apprehended, is the burden imposed upon the heart by the obstructed circulation in the lung or lungs. We cannot directly relieve this, as Dr. George Harley has lately done the parallel condition in the liver by hepatic phlebotomy.¹ But the next best thing can be done, by venesection in the stronger patients, and local bleeding in those of less vigor; seconded in both by early active catharsis and the use of medicines promotive of increased perspiration and urination. Vascular tension and pressure are thus so lessened that the heart may carry its load without exhaustion. The debility of a later stage in severe cases is another thing; but I am obliged to reiterate the assertion that not a few cases of pneumonia in recent times, under prevalent treatment, die during the first week; this being rather conspicuously the case with men of previously good health, and under fifty years of age.²

Under an apprehension of "heart-failure," in pneu-

¹ British Medical Journal, Jan. 15, 1887.

² It is interesting to observe that so eminent an authority as Dr. Osler is recently quoted (Medical News, December 10, 1887, p. 679) as holding that, in typhoid fever, "as it is possible that the defective elimination of the products of regressive tissue-changes may be, in part, at least, the cause of the so-called

monia, many practitioners now resort early to alcohol, in large or considerable doses. Dr. Todd's report of nine typical cases, in his "Clinical Lectures," makes it appear that he was, in his actual practice, more moderate than in his teaching. In that, he advocated the early and free use of alcohol in all acute diseases. There is much evidence that this is an extreme, unjustified, and often injurious practice. Enough for our present purpose to cite the testimony of the physician whom Dr. Todd entrusted with the task of analyzing his records of hospital practice.¹ He asserts that the mortality even from fever, in the hospital attended by Dr. Todd, was in a marked degree greater than that of any other hospital in Great Britain.

Broken-down patients, in or out of hospitals, and those who have been intemperate, may, when attacked with pneumonia, or any other severe disease, be expected to bear and often require active stimulation. But the considerations urged in this paper are intended especially to include and apply to the treatment of simple acute pneumonia, in patients not over middle age, and of previously good health. I think evidence has now been given, sufficient to show that pneumonia is a much more fatal malady than it was forty or fifty years ago; and that the coincidence of this increased mortality with a prevailing change of treatment, gives strong probability to the opinion, that the principles

typhoid symptoms, every effort should be made to keep active the skin and kidneys." This is excellent practice; but we see it added, on the same page, that "constipation may be disregarded, unless it persists longer than seven or eight days" Is seems to me to be hardly an antiquated idea that the bowels, as well as the skin and kidneys, aid in eliminating the products of regressive tissue-changes; and, however it may be in regard to typhoid fever, this eliminative function ought to be utilized in pneumonia, at least in the early stage.

¹ British and Foreign Medico-Chirurgical Review, October, 1860, p. 331.

lying, consciously or unconsciously, under that change of treatment, are erroneous, and ought to be modified.

Objectors to this view, and to the method of its advocacy in this paper, may very readily say that it is antiquated and obsolete. But, when *facts* are allowed to become obsolete, there is an end of science. No sensible person can urge the rejection of any opinion merely because it is old, or the acceptance or retention of any method of practice simply because it is new. *Something* might be expected to be settled by the accumulated experience of the profession, and handed down safely among its traditions, in a true archiatric succession, through our predecessors. Let us make what discoveries we may, and brush away all absolutely exploded notions. But scientific caution would enjoin much care in throwing aside or reversing principles of treatment commended by the testimony of ages. Among those which have best stood the test of time, and which ought, therefore, to be now, according to my judgment, restored to the place in practice from which they have of late, for a time, been excluded, are the principles of moderate early sedation and elimination, instead of stimulation and narcotism, in the treatment of acute inflammatory diseases, of which pneumonia is a type.

There is more room for question, on the basis of experience, between the treatment of forty years ago and pure expectancy, nursing the patient in bed without medication, than there is between the old practice and that which is now generally current.

Such is the conviction, forced upon me by long-continued observation of the field of medical practice, which I have thought it my duty to lay respectfully before the members of the profession.





